note on quantitative predictions

Our analysis is scrupulously independent of scale & notation used to express the result. Thus for a scale with radix Ω in which result is expressed with n_Ω digits we replace $\varepsilon^n \to \left(\frac{1}{\Omega}\right)^{n_\Omega} = \frac{1}{\Omega^{n_\Omega}}$

(a better picture ~~$\frac{1}{\Omega_n - 1} \cdot \frac{1}{\Omega_\Omega^{n_\Omega+1}}$~~ $\frac{1}{\Omega-1} \cdot \frac{1}{\Omega^{n_\Omega - 1}}$)

But best upper bound to number

$$X = a_0 + a_1 \Omega + \dots a_n \Omega^{n-1}$$

expressible in form Ω^N is Ω^{n_Ω}.

So identifying approximately best upper bounds in this sense we have

$$\Omega^{n_\Omega} \simeq \text{const independent of } \Omega.$$

Synthese Methodologies: Bayesian & Popperian.
Vol. 30 Nos 1/2 1975.

Very good article by Agazzi anti Bayes
Richard Jeffrey expounds Bayes.
P(H) may be very small – what we
are alone interested in is $\frac{P(H/E)}{P(H)}$
not absolute value of P(H/E)

Giere cites Neyman-Pearson as equivalent to Popperian methodology.

Miller replies to Jeffrey: Priors are
meaningless subjective, of no practical
use, since cannot be known, as it
priori, not objective.

Miller very interesting paper on the
accuracy of predictions – shows how
accuracy orderings can be "reversed" by
redefining new parameters – e.g. parameters
equivalent to the old parameters
are logic (cf Newman's paradox)

Miller's reply to Jeffreys' [illegible]
[illegible] comment

"Popper's theory – that our decisions are the more rational the more they are subjected to severe criticism (in the light of theories which are themselves severely tested) – can do what Bayesianism cannot do. It makes clear what is rational about a rational decision.

Jeffreys doubts this is more valuable(?).
– But really it is the Bayesian who [illegible] [illegible].

Jeffreys criterion for degree of confirmation

$\frac{p(H/E)}{p(H)}$ not good $= \frac{p(E/H)}{p(E)}$

~~gives~~ confirms H and N iff E confirms H
but E is irrelevant to N.

{ doesn't do relevance of
[illegible] Newton + [illegible]
E = orbits of planets.

He prefers $p(H/E) - p(H)$

He rejects $\frac{p(E/H)\,p(H)}{p(E)} = p(H/E)$

Synthese 1975

Jeffreys considers also likelihood [illegible]
should be rejects – (only allow as potential
[illegible] [illegible] $p(E/H) = 1$. ?)

A. Musgrave?

Scientists work on hard cores they do not believe in   cf Newton / Bohr }

(quoted approv. by Howson)

Difference from Tom on ad hoc.

Tom says ad criterion is implausibility.

quite opposite to my view – Vlock – Nahel Bayesians

$P(C) = 0.95$

$P(B) = 0.5$

Hacking I. Phil Sci. 34 (1967) p 311– 325.
"Slightly more realistic Personal Probability"

discusses dynamic assumptions

claims distinction between conditional prob. $p(h/e)$ and probability given a fact $p(h/e)$.

The Dutch Book argument can justify [illegible] the dynamic assumption.

"Conditional probability indicates how confident a person thinking of the judges that he would be if he knew e as well"

But if he finds e he can then change to a new probability given that he knows e.

Hacking says probability dynamics is a neglected subject – refers to Jeffrey, Hacking and Teller in [illegible]

Hacking argument against conditionalization is rejected by P. Teller (following Lewis and Putnam)
See Synthese 26, 218 (1973)

Paul Feller's views are supported by
William Harper in Synthese 30 (1975) 221-262.